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The chemistry of growth

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Despite the effort expended over many years on understanding it, economic growth remains a mystery. And the harder you look, the more complicated it becomes

ECONOMIC growth is notoriously the blackest of the many black boxes in economics. Familiar as the “stylised facts” of growth may be, attempts to account for them with a single theory have had mixed results, at best.

The dominant family of theories is still organised around the “neoclassical” model of growth first devised by Robert Solow of MIT more than 40 years ago. These theories have the property that a sustained increase in investment increases the economy’s growth rate only temporarily: the ratio of capital to labour goes up, the marginal product of capital declines and the economy moves back to a long-term path, with output growing at the same rate as the workforce (quality-adjusted, in more recent versions) plus a factor to reflect improving “productivity”. Because this last term is exogenous, meaning determined outside the model, critics say that the neoclassical theory of growth ignores the very engine of growth.

The other main approach, which has also spawned an extended family of models, goes by the name of “endogenous growth” theory. The idea was to bring improvements in productivity, notably due to innovation and to investments in human capital, fully inside the model—so that ongoing economic growth emerged as a natural consequence. One line of attack is based on the idea of “externalities” in investment; another concerns itself with the variety of additions to capital (as opposed to mere increases in quantity); yet another tries to take seriously Schumpeter’s ideas (hitherto neglected in mainstream economic thought) on obsolescence and “creative destruction”. Some of these departures have now been “promising” for a suspiciously long time.

The battle between augmented-Solow and sons-of-endogenous-growth rages on. It is a technical debate; overexposure can lead one to forget why growth matters in the first place. A necessary corrective, from time to time, is to read about growth in the real world, and especially in a particular industry. For this purpose, you could not hope to improve on a new book edited by Ashish Arora (of Carnegie Mellon University) and Ralph Landau and Nathan Rosenberg (both of Stanford).

Open the box

Their new collection of papers, “Chemicals and Long-term Economic Growth” (published by Wiley-Interscience) is the latest in a series originating in the Stanford economics department aiming to bridge the gaps between economic theory, economic history and the application of technology. Ralph Landau, a chemical engineer by training and first profession, has been a hugely successful innovator in his own right: he co-founded Halcon-Scientific Design in the 1940s, a company that went on to develop nine major petrochemical processes and build 300 plants around the world. (Far be it from this column to ask whether going from that to being an economics professor is progress.) And Nathan Rosenberg is the co-author of “How the West Grew Rich”, a fine popular history of economic growth, and an authority on the economics of innovation. So the team has lots of human capital.

Much as one feared, however, their book is splendid because it is full of fascinating detail and analysis, not because it settles anything. It includes essays on the history, finance, economics, politics, and science of the global chemicals business—all with a view to explaining its patterns of growth. The industry is well chosen for

such a study, because innovation and the commercialisation of technology are the centre of its own history, and because the business has played a pivotal role in the industrial and post-industrial revolutions of the richest economies. The editors and contributors duly search for big enlightening generalisations about what works and what does not.

If they had found some, ground in the battle between the theories might have been won or lost—but, by and large, they did not. The exceptions are not conclusive; they are mainly of the kind that nobody should find very surprising (such as, “History matters”).

Many interesting anti-generalisations do emerge, however. One is that sources of competitive strength are never constant for long—making the ability to adapt to such changes a crucial talent. In America, for instance, a magnificent endowment of raw materials was decisive early on. Later, size of market and technological prowess mattered more. Later still, as the industry matured, the skills that counted moved back somewhat from science (where they had rested for a time) to engineering, as growth and profits came to depend less on product innovation and more on process ingenuity.

America’s strength has been sustained over decades because it has successfully managed these transitions from one source of advantage to the next—rather than resting on, or trying too hard to entrench, the advantages that it started with. The contrast with Germany (in some respects) and with Britain (in many) is sharp, and described in several chapters.

But the problem with identifying adaptability as a key is of course that this virtue is itself multi-dimensional and averse to simple generalisations. Why has America been more adaptable than Britain? Because of its science-education system? Because its society values engineering over ancient Greek? Because its financial system proved better at supporting new companies and restructuring failing ones? Because of a broader culture that tolerates (even esteems) business failure—and the temporary dislocation and unemployment that go with it? Because unions have been less powerful? Because governments have been less intrusive? Yes, yes, all of the above.

Which is all very well, but what matters most? What is the main thing governments must do to spur economic growth? Ah, well, that remains a mystery.

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